Der Panamakanal. By Max D. Fiegel. Pp. viii+183. (Berlin: Dietrich Reimer—Ernst Vohsen, 1911.) Price 4 marks.

MR. FIEGEL describes clearly the course of the canal now in process of construction by the United States Government; also the engineering works and machinery, and the commercial and political aspects of the enterprise. His book will provide German readers with an informative account of the position about a year ago of what in three years' time promises to be one of the most remarkable human schemes ever brought to a successful conclusion.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Weather of 1911.

SIR EDWARD FRY'S letter in NATURE of November 16, and Commander Hepworth's reply in the issue of November 23, prompt me to give expression to some ideas which I have been discussing with various meteorological friends during the last few months. What I have to say will not attempt an explanation of the phenomena of the remarkable summer and autumn of this year, but it will give an indication of the direction in which, for my part, I hope to look for an explanation of those phenomena.

I agree with Sir Edward Fry that the prevalence of anticyclones in a particular direction, or the continuance of given winds, are only part of the phenomena to be explained; I think, too, that the statement may be rightly understood in a sense slightly different from that which Sir Edward intended. I mean that this summer furnished a good deal of evidence for a proposition which includes the inference that the distribution of pressure is not in itself a complete explanation of the weather. I will state my proposition now, and afterwards explain why I make it. It is that the main outlines of the distribution of pressure are imposed upon the surface layers of the atmosphere by transmission from a region 9 or 10 kilometres high—a region which is above what may be called the physical laboratory, where rain and thick clouds are made—and that the phenomena of weather are due, not to the mere existence of the air currents which correspond with the distribution of surface pressure, but to their heterogeneity. Weather as represented primarily by rainfall is dependent on the convection of moist air, while pressure distribution is governed by changes which take place above or nearly at the top of the convective region of the atmosphere. In other words, the dynamics of the atmosphere is controlled in the upper air, while the physics of the atmosphere is a matter which concerns the lower layers.

The remarkable summer has provided evidence in support of this proposition by furnishing a number of examples of pressure distributions which might well have been rainy, and were not. The Coronation festivities were somehow preserved from the copious rainfall which, according to the pressure distribution, was their due. If pressure distribution is the cause of rainfall, 100,000 children at the Crystal Palace on June 30 ought to have got wet through, but they did not. There are many other instances of the same kind which I need not quote. Let us look at the matter from the other side.

Some time during the summer Mr. W. H. Dines sent to me the results of some work which he has done upon the correlation of various data for the upper air. Among them was the correlation coefficient between the variations of pressure at the surface and at the level of 9 kilometres. For certain groups of ascents it was so large as to show a close approximation to proportionality. That is, of course, not surprising, because the variations at the surface and at 9 kilometres are certainly not independent; but what was surprising to me was that the standard deviations of pressure at the two levels in the cases under investigation were very nearly equal. Thence it follows that the pressure variations at 9 kilometres level (with two-thirds of the atmosphere below it) are not merely proportional to the variations at the surface, but the same in magnitude; and as they are certainly transmitted to the surface, it follows, further, that the variations at the surface are practically accounted for by the variations that occur at 9 kilometres. We are accustomed to urge the importance of the study of the upper air for increasing our knowledge of meteorology; but, so far as I know, we have not recognised that it was so directly responsible for steering our surface air currents.

Mr. Dines's variations were those shown between the individual balloon ascents and their average. Looking into a series of charts for the upper air recently published by Prof. Rotch, I came upon another step in the proof. In the charts Prof. Rotch gives the average wind velocity at 30,000 feet (9 kilometres) and the atmospheric density there. The product of these two is about the same as for a point near the surface, whence it follows that for Blue Hill the pressure gradient at 9 kilometres, which is proportional to the product, is the same as for the surface; in other words, not only are the chronological changes transmitted to the surface from 9 kilometres, but the average pressure distributions are similarly transmitted. In a discussion at the Meteorological Office on October 23 I was reminded that these conclusions are not new. The inverse proportionality of velocity and density of air is known as Egnell's law; and the approximate constancy of pressure gradients up to 9 kilometres was pointed out to the British Association by Gold and Harwood in 1909. But the idea of looking to the level of 9 kilometres for the outlines in full scale of our surface distribution of pressure is new; and it seems to me to be possibly the beginning of a new era in the endeavour to explain such phenomena as those of the past summer.

I cannot, at this stage, give particulars as to the details of the application of so general a proposition to special cases, such as circular revolving storms, the northern sides of which may be confined to the lower strata, nor can I say whether the application of the proposition is limited to certain parts of the world. I think it must be. It will be remembered that M. Teisserenc de Bort computed mean isobars at 4 kilometres for January and July that showed the average circulation of the upper air in each hemisphere as a great cyclonic depression, with centres at the poles. It is not likely that there is any great change of distribution between 4 kilometres and 9 kilometres. At Blue Hill the winds at 3 kilometres vary between W.S.W. and N.N.W., and these, again, should agree with the pressure distribution. We know from the study of the points where sounding balloons land that the westerly circulation is not always to be found aloft. We know, also, that at 9 kilometres the variations of temperature from day to day are as large as, or larger than, those at the surface. Hence we may conclude that the pressure distribution at 9 kilometres corresponds with a cyclonic circulation of westerly winds round the pole, periodically, but perhaps not regularly, invaded by winds from some northerly quarter, with marked changes of temperature. This will be recognised as merely a rough description of a series of **V**-shaped depressions, which also, on the average of the month, would give a westerly circulation. Possibly, in reality, the **V**-shaped depressions at 9 kilometres are made up of comparatively warm westerly winds with repeated incursions of cold air from the north or north-west, giving phenomena similar to those which have been described in recent discussions of line squalls. In the results of Mr. Cave's observations of pilot balloons there is evidence that **V**-shaped depressions at the surface sometimes originate with northerly winds at high levels.

If this is so, our failure to explain the outlines of the distribution of pressure by means of the surface conditions is itself explained. Mr. Dines's recent paper before the Royal Society shows how futile is the endeavour to explain them by reference to temperatures in the layers below 9 kilometres. They come from above, and their shape at any time is governed by causes in the consideration of which we must treat the globe as a whole. Our first step in explaining, for example, the recent succession of gales would be to note whether the westerly circulation in the

region of cirrus clouds had become greatly intensified. We may infer that that circulation depends, at least, in part on the differences of temperature at different latitudes, because the winds are strongest in the winter, when the difference of temperature between the tropics and the pole is greatest; but we cannot yet describe the mechanism of the process nor the variations from year to year. On the other hand, rainfall seems to have to do rather with the small variations of pressure, which elsewhere I have called the embroidery of the barogram, than with the main features of the barogram. Considerations of space prevent my pursuing here the suggestions that this proposition entails. I need hardly say that the subject is not exhausted by what I have said. W. N. SHAW.

November 27.

The Inheritance of Mental Characters.

MR. WALKER sent me, before publication, the letter which appeared in NATURE of November 23. In reply I explained that, though I have insisted elsewhere that the words *inborn*, *acquired*, and *inheritable* are often incorrectly used, yet in my paper to the Eugenics Education Society I did not define the meanings of them, partly because my space was limited and partly because I thought no misapprehension could arise in the minds of my audience. None did arise. But I learn, with surprise, that some would have arisen had my critic been present. I used the words exactly as they are commonly used in biological literature, terming such characters as heads and instincts inborn and inheritable, and such characters as scars and a knowledge of Latin acquired.

In Mr. Walker's book, "Hereditary Characters," he was good enough to reproduce many of my conclusions almost in my own words; for example, "In considering the mental characters of man we are forced to the conclusion that almost all are acquirements, and that very little besides consciousness, memory, capacities for making various acquirements, and a few instincts is inborn." This is precisely my opinion as elaborately set forth, not only "on all previous occasions," but in the very paper he criticises. The astounding thing is that he should imagine that it is, or may be, also the opinion of Prof. Karl Pearson, whose statement, he thinks, "may be loosely expressed and open to misinterpretation," but "which does not appear, on the face of it, to be at variance with his own views." I fear Prof. Pearson will pray ardently to be delivered from his friends.

I must complain that the sentence Mr. Walker quotes from my paper is, in the absence of its context, open to misinterpretation. I was not merely railing. The following is the passage from which it is taken. Prof. Pearson does not use the word "inborn"; but if the word "inheritable" or "physical" be substituted for it, my meaning is unaltered

"... Here we have an example of a conclusion based, like many more of the conclusions of biometricians, on an ascertained correlation. It is assumed that, since offspring reproduce parental mental characters in the same degree as their physical characters, therefore, if the latter are inborn, the former must be inborn also. In other words, it is assumed that one kind of sameness necessarily involves another and a different kind of sameness."

Now, though I have collected no family histories bearing on the subject, I think that no one will deny that such characters as heads, hearts, lungs, livers, and the like are inborn and invariably present in parents and offspring—at any rate in offspring that reach school age. Here we have absolute certainty of inheritance. Again, I think no one will deny that parental birth-marks, moles, and the like, are also inborn, and that they are hardly ever, if ever, reproduced by offspring. Here the degree of inheritability is zero. Between these extremes of inheritability lie the degrees of inheritability of all the other characters. Some, like ten fingers and ten toes, are reproduced almost as certainly as heads; others, like eye-colour and hair texture, with less certainty; others with still less certainty; and so on, and so on, until we reach characters the inheritability of which is scarcely greater than that of birth-marks and moles. Plainly, then, since the inheritance of inborn characters varies between certainty and zero, Pearson's statement is without significance—void of all content. Founded with such an air of scientific accuracy on statistics and family histories which have such an appearance of scientific precision, it is so vague as to be quite nonsensical. By the use of his method any character you like may be "proved" to be inborn; for, if only you seek conscientiously, you will be sure to find another undoubtedly inborn, which is reproduced by offspring in about the same degree. Thus all English children have heads, and all speak English. If, then, you are satisfied with the method, you may conclude that English speech in English children is "bred, not created." Or, alternatively, that English children acquire their heads—for you are happy in always having these alternatives to choose from.

G. ARCHDALL REID.

Southsea, November 26.

Amedeo Avogadro.

EXACTLY a century has passed away since the eminent Italian physicist Avogadro published the law which, bearing his name, is now familiar to every elementary student of chemistry or physics. Owing to various causes, Avogadro's labours were but little appreciated, and though he occupied a foremost position among Italian men of science the scientific world in general evinced but little interest in his work. Even the indefatigable Kopp failed to realise the importance of his speculations, with the result that his first history of chemistry contained no reference at all to them. Like Carnot's, Avogadro's writings had to await an interpreter and supporter. In this respect, however, fortune proved more than kind, and in his countryman Cannizzaro, Avogadro obtained an illustrious disciple and a brilliant exponent of his doctrine. But while it is true that the name of Avogadro is now widely known, it is no less true that the record of his life seems to have escaped biographers and historians alike, a statement which will be substantiated by a search through the ordinary English works of reference.

Count Amedeo Avogadro di Quaregna came of a distinguished and noble family of Biella, a small town some way north of Turin, in Piedmont. Various members of the family had won fame in the courts or on the field, but the name is unknown to the world at large except through the work of this student of physical science. Amedeo's father, Count Filippo, married Anna Vercellone, of Biella, and on August 9, 1776, their son was born at Turin, the birthplace of Lagrange. In obedience to his father's wishes young Avogadro studied law. He received his diploma on March 16, 1796, and during the next few years occupied positions in various branches of the law offices. In April, 1801, he was appointed secretary to the Prefecture of the Eridano province. His natural inclination for mathematics and physics, however, led him to carry on his studies, and with his brother Felice he wrote, and presented to the Academy of Sciences of Turin, two papers, in 1803 and 1804, the first being on electricity and the second on the nature of metallic salts. For this work the brothers were nominated corresponding members of the academy on July 5, 1804. Amedeo now obtained permission from his father to follow the career of his own choosing, and after a short course of study he became in 1806 a demonstrator at the Royal College of the Provinces. Three years later, on November 7, 1809, he was appointed professor of positive philosophy (physics and mathematics) in the Lyceum at Vercelli, where he remained until 1821. It was during the early years of his residence at Vercelli that he produced the two memoirs which have immortalised his name. Both the memoirs were published in the *Journal de Physique* of De la Methérie. The first, which appeared in 1811, was entitled "Essai d'une manière de déterminer les masses relatives des molécules élémentaires des corps, et les proportions selon lesquelles elles entrent dans ce combinaison"; the second appeared in 1814, and was entitled "Mémoire sur les masses relatives des corps simples, ou densités présumées de leur gaz," &c. Avogadro returned to the same subject in a memoir which he published in 1821, 1826, and in 1849, and he investigated other problems bearing on the same subject. His other researches included questions in electricity, chemistry, electrochemistry specific heat, and the expansion of bodies.